COMMENTS ON RASNITSYN'S PROPOSAL TO REGULATE THE NAMES OF TAXA ABOVE THE FAMILY GROUP. Z.N.(S.)2381

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Rasnitsyn's (1982, *Bull. zool. Nom.* vol. 39, pp. 200–207) formal proposal to change the International Code of Zoological Nomenclature so as to govern names above the family group level by extending the rules applicable at the lower levels, as proposed by Rohdendorf (1977, *Palaeont. Zhurnal.* vol. 11, pp. 14–22), needs thorough consideration. Although such proposals appear useful at first sight, there are various drawbacks. I do not intend to go into these in any detail here, but will merely indicate some of them. A more extensive discussion will be found in Brothers (1983, Nomenclature at the ordinal and higher levels, *Syst. Zool.* vol. 32, pp. 34–42).

Despite the statement in the proposal that 'the rule of the nomenclatural type . . . needs no comment,' this is the main problem. The use of type specimens at the species level is certainly indispensable. Type species at the generic level are also essential because of the requirements of binominal nomenclature. The use of type genera at the family group level is useful but can be confusing because the limits of a taxon designated by a particular name may change according to the opinions of the systematist. It is thus not obvious which group is meant when reference is made to 'VESPIDAE', for example—this may or may not include EUMENINAE, MASARINAE, etc.—unless the name is qualified by a statement such as '*sensu* Richards, 1962.' The problems which will result from application of a type concept at the higher levels are similar but will be much more extreme.

The reason for the confusion is that application of a type system confounds classification (subjective) and nomenclature. Ideally, nomenclature should be entirely objective such that a particular taxon (comprising particular subgroups) should always bear the same name and, furthermore, a particular name should not be applied to more than one taxon. Only under such conditions will it always be obvious exactly which group of organisms is involved when a particular name is used. Under the system proposed by Rasnitsyn and Rohdendorf a particular taxon will have different names depending on whether it is recognised at the ordinal or subordinal level, for example. Admittedly, the differences will be merely in the suffix, so that minimal confusion may result, but the names will still be different. On the other hand, the same name may very well be applied to different groups, depending on the opinions of the systematist as to whether a larger or a smaller grouping should be recognised at a particular categorical level. Thus, using a type system means that a particular group may have more than one name and also that a particular name may be applied to more than one group. I am thus convinced that the use of a type system at the higher levels will promote confusion rather than clarity and have suggested an alternative approach in my paper referred to above.

A further difficulty with Rasnitsyn and Rohdendorf's proposals, and one which should not be underestimated, is that their adoption will necessitate the abandonment of all names presently used for higher taxa and their replacement with names which will be very strange to many workers. This may not be an extreme problem to the systematists actually working on each group, but it will drastically affect non-taxonomically oriented biologists. I thus anticipate that such rules are likely to be honoured more in the breach than the observance, as is already the tendency at the family group level. ('It involves too much time and effort to track down which author first used a name based on a generic name within the family group—systematists have more important things to do!' is a common argument). If rules are to be proposed then they must be such that most biologists will honour them. I suspect that any rules governing higher taxa, especially ones requiring extensive name changes, will not be so honoured, and it is for that reason that my proposals referred to above were put forward as principles and not as rules.

COMMENTS ON THE PROPOSED CONSERVATION OF *GALEOPSOMYIA* GIRAULT, 1916. Z.N.(S.)2402

(see vol. 39, pp. 297–301)

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(1) In their paragraph 3 the applicants state that Nowicki, 1929, designated *Trichaporus aleyrodis* Mercet, 1930, as the type species of *Trichaporus* Foerster, and *Trichoporus melleus* Ashmead, 1904, as type species of *Trichoporus* Ashmead, 1900. Furthermore, they state that this practice was generally accepted by workers since 1929. In their paragraph 5, however, they state *Trichaporus* Foerster sensu Nowicki is a senior synonym of a generally accepted name, *Encarsia* Foerster; and that *Trichoporus* Ashmead sensu Nowicki is a senior synonym of the widely used name *Galeopsomyia* Girault. The wide use of *Encarsia* and *Galeopsomyia* seems to be in contradiction to the general acceptance (after 1929) of their respective senior synonyms *Trichaporus* and *Trichoporus* sensu Nowicki. Can they explain this?

(2) If Ashmead, 1900, when using the spelling *Trichoporus* for the generic name *Trichaporus* Foerster, 1856, did not indicate expressly that he intentionally changed Foerster's spelling, *Trichoporus* is not an emendation but an erroneous subsequent spelling and consequently has no status. Therefore there is no reason to suppress it. The fact that he used *Trichoporus* several times in his paper does not show at all that it is intended as an emendation. He may have misread the word.

In 1949 Miss Buitendijk and I published a paper on a crab the name of which we cited as *Rhitropanopeus* Rathbun, 1898. We used that spelling consistently and frequently in the paper. Later it was pointed out to us that the correct spelling is *Rhithropanopeus*. I can emphatically state that our spelling was not an emendation but simply an error.